The Influence of Temperature on the Action of Nitric Acid on Metals.

THE following simple but striking experiments illustrating the influence of temperature upon the action of nitric acid on metals may possibly be of interest to those who are engaged in the teaching of chemistry.

If three tubes containing strong nitric acid are cooled below $-10°$ C. by means of a freezing mixture of snow or pounded ice with salt, and then copper turnings added to one, granulated zinc to another and magnesium ribbon to the third, it will be found that no action takes place, the nitric acid being practically inert at this temperature. If the tubes are then exposed to air at about 22° C. so that the temperature rises slowly, it will be found that little or no action occurs until a certain temperature is reached, when a sudden and violent ebullition of brown fumes occurs, the metal rapidly dissolving and the temperature abruptly rising from 80° C. to as much as 104° C.

The critical point for this violent action lies in the case of zinc between 0° C. and 2° C., in that of magnesium between 17° C. and 19° C., and in that of copper between 19° and 21° C. Before these temperatures are reached very feeble action may occur and a few bubbles of gas be disengaged, especially in the case of the zinc. These bubbles consist partly of hydrogen gas, and if magnesium is added to cold dilute ($\frac{1}{6}$–$\frac{1}{8}$) nitric acid an active evolution of nearly pure hydrogen takes place at first, although as the solution becomes warm and the percentage of magnesium nitrate increases, the production of hydrogen rapidly diminishes. This is in somewhat striking contrast to the common statement in chemical text-books to the effect that in no circumstances can hydrogen be obtained by the action of nitric acid on metals.

ALFRED J. EWART.

Meteorological Work for Science Schools.

IT has often occurred to me that the collection of data, such as those necessary for the investigation of fog distribution, might well be entrusted to the science schools over which the Technical Education Board of the London County Council exercise control.

There is, in such a research, that element of originality which is needed in the work of our school laboratories.

For interpretation the collected data may afterwards be distributed to the schools engaged in the work.

I foresee only the difficulty due to the intervention of vacations.

J. V. H. COATES.

41, East Dulwich Grove, S.E., November 25.

[We have referred the foregoing letter to the Secretary to the Meteorological Council, who has been good enough to send the following remarks upon it.—Editor, NATURE.]

THE primary difficulty in the way of using science schools, as suggested by Mr. J. V. H. Coates, for the immediate purposes of such an inquiry as that of the distribution of fogs is that the schools have fixed hours of attendance to which the fogs pay no heed. To carry out such an investigation effectively the twenty-four hours must be taken into account. Of course the inquiry might be restricted to those fogs which begin or end within the hours of attendance, but that would be a very serious limitation. As confirmatory evidence, careful observations within school hours might be very useful. The necessity for securing a suitable uniformity among observers as regards the terms employed in the estimation of fogs makes it necessary, however, to proceed with caution in extending the number of separate observers.

The kind of cooperative investigation which is appropriate for organised science schools is one which can be dealt with primarily by observations at fixed hours. On special occasions it might doubtless be pursued beyond those hours in following up some definite point. Several inquiries of this nature may be suggested. For example, in relation to the fog inquiry, it is desirable to know something of the effect of wet ground during rapid falls of temperature. For this purpose an investigation of the temperature of wet soil or sand suitably exposed and its relation to the temperature of the air would be a very useful adjunct to the ordinary meteorological data. It is a part of the inquiry more suitable for science schools than for routine observation, because the conditions of exposure require examination and consideration as well as the readings of the thermometers. The hours of non-attendance could be bridged by registering minimum instruments or, in some enterprising schools, by self-recording instruments, the development and investigation of which would be in themselves a useful study.

Another line of cooperative inquiry, of much greater difficulty, suggested to me in various forms by several scientific friends, has reference to the large amount of fuel consumed daily within the London area. The combustion must of necessity raise the temperature of the air in or over London above that of its surroundings. The raised temperature should give rise on calm days to a diminished pressure and an inflowing air current. Ordinary meteorological observations are not of a sufficiently high order of accuracy to exhibit these effects, but by cooperative, and in the best sense competitive, effort between science schools in different parts of the metropolis progressive steps could be reached which might ultimately have the very satisfactory result of exhibiting quantitatively the effects of the local heating. If this ultimate purpose should not be achieved, the light thrown upon the practicable limits of observation would not be without interest.

Then, again, the chemical composition of air at different points during foggy days would be a useful inquiry. Probably the results obtained at the first attempt would not be accepted as final, but the discussion of the results from different centres would lead to more accurate determinations and ultimately to definite information of substantial value. Incidentally, such cooperative inquiries would be of very great educational influence and advantage. Supposing, for example, that it were decided to make observations of any rapidly varying element at a definite point of time, the mere carrying out of the comparison of the time-keepers at the different schools would be most instructive. The comparison of their length-standards with a view to accurate barometric measurement might be beyond the reach of available apparatus, but even the demonstration by appeal to experience that the best comparisons that could be effected with the apparatus at command, left a margin of inaccuracy of a certain defined magnitude would be sufficiently instructive to make the experiment worth trying.

W. N. SHAW.

November 30.

The Date of Stonehenge.

THE remarkable paper on Stonehenge, by Sir N. Lockyer and Mr. Penrose, in NATURE of November 21 has greatly interested me. Just two years ago I was working at the subject, and wrote to Prof. Petrie to inquire what azimuth he had used for the axis of the temple in his estimate of the date, which he gives as 730 A.D. ± 200 years, with a possible date of 400 A.D. As I received no reply I employed the angle 50° 12′ E. of N., given in Mr. Edgar Barclay's "Stonehenge," 1895. With this azimuth I obtained by means of a formula, kindly supplied by Dr. Downing, F.R.S., a date of 425 A.D. I find that, for the given azimuth, even this date is too early, as I did not allow enough for refraction, &c. Applying the same formula to the figures given in Sir N. Lockyer's paper, the date comes out about 1700 B.C., as stated, so that the formula was correct, and the chief error was in the erroneous azimuth of the axis, which differs by about 38′ from the 49° 34′ 18″ so carefully determined in the published paper. Now as an increase of some 90″ in sunrise azimuth at the solstice means a decrease of some 46″ in declination and represents the lapse of about a century, the discrepancy is clearly explained. Allowing for refraction, &c., I make the present azimuth of the sun at sunrise at the solstice about 50° 26′ 21″ E. of N., the sun's declination being 23° 27′ 8″ N. Consequently since the date, 1700 B.C., the solstitial sunrise azimuth has shifted 52′ 3″ further E. and the declination has decreased 27′ 22″, representing a lapse of about 3600 years, when the appropriate formula is applied.

At the distance (250 feet) of the Friar's Heel, or Sun-stone, from the centre of the ruin, a change in azimuth of 52′ would shift a point on the axis only 3 feet 9 inches, and, as the avenue is 50 feet wide, some idea may be formed of the necessary delicacy of the measurements. The azimuthal shift of the sun himself is less than two diameters. It seems to me very improbable that any estimate of the date closer than that arrived at by Sir N. Lockyer and Mr. Penrose can be made on astronomical grounds. Recent excavations have given valuable information, but much more yet remains to be done in this direction. I may add that an exhaustive study of the "Blue-stones" (igneous rocks foreign to the locality) by the methods of modern petrology may lead to

some definite knowledge of their origin and so throw fresh light on the whole problem. C. T. WHITMELL.

Leeds, November 23.

P.S.—For sunrise (in accordance with p. 57) I take the tip of the visible sun to be 2′ above the local horizon.

Change of Pitch in certain Sounds with Distance.

SEVERAL years ago the late Prebendary Simpson, of Fittleworth, Sussex, told me of an interesting observation he had made, which some of your readers may be able to explain. While walking up and down the platform of a railway station, he noticed a peculiarity in the sound of a steam jet from an engine standing on the lines. The pitch of the sound appeared to rise as he retreated from the engine and to fall as he drew near to it. Some time after, Mr. Simpson observed the same thing again, but in this instance the noise was made by a gas flare in the open air, about which some men were at work. Since then I have found that this alteration of pitch with distance occurs with any fizzing noise of the kind, such as that of air jets, burning logs, frying fat, pouring rice or coffee beans, waterfalls, or even the rustling leaves of a single tree; with all those noises, in fact, whose sources are sufficiently localised to admit of observations of the kind being made. I found, also, on withdrawing from such a source that a point is reached after which the pitch ceases to rise, and remains practically stationary as far onward as the sound continues audible. This point is sometimes pretty definitely marked, and varies in distance from the source with different sounds, and the pitch of the stationary portion also varies in the same way. I do not think, however, that the pitch of the whole volume of sound changes, though it often appears to do so, for a similar impression is created by moving a fizzing air jet to and fro close to a wall. As it nears the wall, the whole sound seems gradually to rise in pitch and to sink again as the jet is withdrawn. But here the effect is clearly due to successive reinforcement of one part of the noise after another in the order of their wave-lengths. It is only a shifting of the point of greatest intensity, and not an actual change of pitch at all. Assuming, then, that the effect noticed by Mr. Simpson is of the same nature, that is to say, caused by a readjustment of the relative intensity of the parts, how is it to be accounted for? Is it simply a process of *sifting* by distance, the weaker groups of small noises, of which the fizzing sound is composed, dropping out of earshot in succession, as the observer retires from the source, till only the largest and loudest group is left, which last continues to be heard for the remainder of the distance without sensible change of pitch? If that is so, then the deeper tones of such noises would seem to have a proportionally shorter range of audibility than the higher ones; for, so far as I have observed, the pitch always sinks on approach to the source and rises on withdrawal from it, never the reverse way, as might be expected in the case of very bass roaring sounds. Perhaps, however, others may have noticed instances of the latter sort. The behaviour of the air-jet fizz at the wall illustrates a kind of reciprocal action, which no doubt plays an important part in the adjustment of the pitch. The tone which is loudest for the moment appears to dominate and obscure the rest, so that, near the source, where the deeper tones are most powerful, these latter, to some extent, subdue and lower the principal one, while further off, where they become enfeebled by distance, they are in their turn still more diminished by the presence of the principal far-reaching tone.

Downshire Square, Reading. FREDERICK M. WEST.

Pine Grosbeak in Berkshire.

IS it not of rare occurrence that a pine grosbeak (*Pyrrhula enucleator*) has been seen here, not on one day, but on two? I was informed this morning that Mr. O. T. Perkins had seen this handsome bird out of his window, apparently either eating beech buds or else hunting for insects on them. During this the bird was attacked by three sparrows, who began making a great noise and eventually drove him off. This morning I saw the same grosbeak, or another one, in a like manner feeding on beech. And what is more strange he was again attacked by sparrows and had to beat a hasty retreat. I may add that the bird, to all appearances, was in excellent condition, its plumage being brilliant. I wonder if any other of your readers have noticed any of these handsome but rare birds? C. M. ROGERS.

Blucher, Wellington College, Berks.

THE "ARMORL" ELECTRO-CAPILLARY RELAY.

WE commented in our notes columns a short time ago upon the announcement that a new system of wireless telegraphy had been worked out by Messrs. Orling and Armstrong. From what could be gathered from the information at that time available we judged that the method made use of earth conduction; we have since learned that this is the case and that the inventors rely upon the novel design of their transmitting and receiving apparatus for the efficiency of their results. We have had an opportunity of inspecting drawings of the receiving apparatus, and are enabled to give a description of it, though we have not seen the actual apparatus itself, but only a working model. We understand that it is proposed to read a paper shortly on the transmitter before one of the scientific societies and that in consequence it is not desired to publish the details of its construction as yet. It is to be hoped that

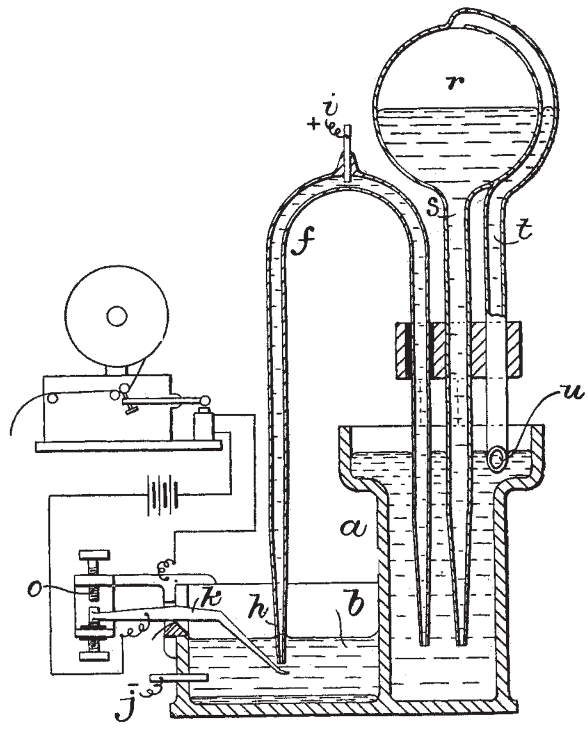

FIG. 1.—"Armorl" Electro-capillary Relay. Syphon form.

at the same time an account will be given of the experimental results obtained, with trustworthy data from which the probable value of the invention may be gauged, for as yet there is nothing to go upon but the statements of the inventors.

In the meantime we must content ourselves with giving a description of the receiver, which is of interest independently of its use with the Orling-Armstrong or any other wireless telegraph, as it could be used for the detection of any sort of electrical current. The instrument consists essentially of a capillary electrometer which is arranged so that it can actuate a relay. The extreme sensitiveness of the capillary electrometer for very small currents and low electromotive forces is well known, and the instrument is used considerably, especially for physiological work. The arrangement adopted in the present instance is shown in Fig. 1. A syphon, *f*, is